THURSDAY, MARCH 26, 1896.

THE SCIENTIFIC CORRESPONDENCE OF GEORGE ROMANES.

The Life and Letters of George John Romanes. Written and edited by his Wife. Pp. viii + 360. (London: Longmans, Green, and Co., 1896.)

THE life of most men of science is uneventful so far as the rest of the world is concerned, and can hardly interest more than the circle of personal friends. And the letters of men of science also have, as a rule, but a limited interest. Any scientific information which they contain has usually been published elsewhere, and if of importance, is already familiar to the scientific reader. The account of the life, and the selection of the letters of George Romanes, which is here presented, offer in some measure an exception to the general rule, for they contain matters which will keenly interest others than those who may desire to follow merely the scientific career of the man. But it is the scientific correspondence which will, without doubt, be that which will chiefly interest the readers of NATURE, and it is this, therefore, with which alone we propose here to deal. And of the scientific correspondence, that which Romanes carried on with Charles Darwin during a period of nine years would alone furnish enough interest to ensure a cordial welcome to this work. For Mrs. Romanes has not confined herself, as is so often done by the biographer, to presenting only the one side of a correspondence, but has wisely decided in this and certain other instances to allow both sides to appear, far more than doubling thereby its value and interest. And since, with one exception, none of these letters of Darwin have hitherto been given to the world, and moreover he seems in them to have opened up his heart with the frankest confidence to the young devotee whose powers of thought he was one of the first to recognise, it is obvious that this correspondence must have an altogether exceptional interest. It deals mainly, as might be supposed, with subjects bearing on the question of inheritance, and especially with Darwin's hypothesis of Pangenesis. In attempts to prove this theory, Romanes devoted an immense amount of time in patient experimentation, but as the experiments yielded for the most part a negative result, showing neither for nor against the hypothesis, they were never systematically published. On this account the descriptions of some of these experiments, which are here given at length, will be welcomed by those who may be carrying on similar work in the future. The subject was one upon which Romanes was "keen" to the very last, and almost the latest experiments which he planned, had a direct bearing upon the doctrine in question. And even when his time was fully occupied with the fascinating experiments upon Medusæ, by which he first found fame as a scientific inquirer, he was able to devote attention to the carrying out of laborious work on this subject. In July 1875, he writes to Darwin:

> As you have heard about the Medusæ, I fear you will infer that they have diverted my attention from Pangenesis; but although it is true that they have consumed a great deal of time and energy, I have done my best to keep Pangenesis in the foreground.

Then follows a precise account of numerous experiments on grafting of tubers and the like, after which he continues—

> But as I am a young man yet (he was just 27), and hope to do a good deal of "hammering," I shall not let Pangenesis alone until I feel that it does not admit of being any further driven home by experimental work; and even if I never get positive results, I shall always continue to believe in the theory.

And a little later—

> I have an idea that you are afraid I am neglecting Pangenesis for Medusæ. If so, I should like to assure you that such is not the case. . . . I confess, however, that but for personal reasons I should have postponed Pangenesis, and worked the Medusæ right through in one year. There is a glitter about immediate results which is very alluring.

To which Darwin replies—

> So far from thinking that you have neglected Pangenesis, I have been astonished and pleased that your splendid work on the jelly-fishes did not make you throw every other subject to the dogs.

In another letter from Darwin the following occurs—

> As you are interested in Pangenesis, and will some day, I hope, convert an "airy nothing" into a substantial theory, therefore I send by this post an essay by Häckel, attacking "Pan.," and substituting a molecular hypothesis. If I understand his views rightly, he would say that with a bird which strengthened its wings by use, the formative protoplasm of the strengthened parts becomes changed, and its molecular vibrations consequently changed, and that their vibrations are transmitted throughout the whole frame of the bird. How he explains reversion to a remote ancestor I know not. Perhaps I have misunderstood him, though I have skimmed the whole with some care. He lays much stress on inheritance being a form of unconscious memory, but how far this is part of his molecular vibration I do not understand. His views make nothing clearer to me, but this may be my fault.

In a letter written in 1877, after referring in enthusiastic terms to a lecture by Romanes on the "Evidences of Organic Evolution," he says:

> I am very sorry to hear about the failure in the graft experiments, and not from your own fault or ill-luck. Trollope, in one of his novels, gives us a maxim of constant use by a brickmaker, "It is dogged as does it!" and I have often and often thought this is the motto for every scientific worker.

With characteristic generosity Darwin handed over to Romanes, who was preparing his book on "Animal Intelligence," his notes on instinct.

> You are quite welcome to have my longer chapter on instinct. It was abstracted for the Origin. I have never had time to work it up in a state fit for publication, and it is so much more interesting to observe than to write.

The book in question was heralded by a lecture on the subject given before the British Association at its meeting in Dublin in 1878. A copy of the lecture, as well as a newspaper account, from which it appeared that the lecture (especially an allusion in it to Darwin himself) was most cordially received, was sent by Romanes to Darwin, who thus acknowledges the receipt:

> I am most heartily glad that your lecture (just received and read) has been so eminently successful. You have indeed passed a most magnificent eulogium on me, and I wonder that you were not afraid of hearing "Oh! oh!" or some other sign of disapprobation. Many persons think that what I have done in science has been much overrated, and I very often think so myself; but my comfort is that I have never consciously done anything to gain applause.

He jocularly adds in a short note sent a few days after—

> Frank says you ought to keep an idiot, a deaf mute, a monkey, and a baby in your house.

To which Romanes rejoins—

> Frank's idea of "a happy family" is a very good one; but I think my mother would begin to wish that my scientific inquiries had taken some other direction.
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> The baby too, I fear, would stand a poor chance of showing itself the fittest in the struggle for existence.

And two years afterwards the joke is continued (the baby having in the meantime put in an appearance).

> I have now got a monkey. Sclater let me choose one from the Zoo, and it is a very intelligent, affectionate little animal. I wanted to keep it in the nursery for purposes of comparison, but the proposal met with so much opposition that I had to give way. I am afraid to suggest the idiot, lest I should be told to occupy the nursery myself.

The following postscript to a letter from Darwin, dated September 14, 1880, will be of interest.

> We went to the Lakes for three weeks to Coniston, and the scenery gave me more pleasure than I thought my soul, or whatever remains of it, was capable of feeling. We saw Ruskin several times and he was uncommonly pleasant.

The postscript to another of Darwin's letters (dated January 24, 1881) is the following parable :—

> N.B. Once on a time a fool said to himself that at an ancient period small soft crabs or other creatures stuck to certain fishes; these struggled violently, and in doing so discharged electricity, which annoyed the parasites, so that they often wriggled away. The fish was very glad, and some of its children gradually profited in a higher degree and in various ways by discharging more electricity and by not struggling. The fool who thought thus persuaded another fool to try an eel in Scotland, and lo and behold electricity was discharged when it struggled violently. He then placed in contact with the fish, or near it, a small medusa or other animal which he cleverly knew was sensitive to electricity, and when the eel struggled violently, the little animals in contact showed by their movements that they felt a slight shock. Ever afterwards men said that the two fools were not such big fools as they seemed to be.

About this time Romanes began to consult Darwin concerning experiments as to the relative effect of flashing and continuous light upon seedlings. These experiments were pursued at intervals during the next ten years, and the results were published in a paper read before the Royal Society in 1892. He tells also of a curious experiment on the sense of direction in cats.

> I have got a lot of cats waiting for me at different houses round Wimbledon Common, and some day next week shall surprise our coachman by making a round of calls upon the cats, drive them several miles into the country, and then let them out of their respective bags. If any return, I shall try them again in other directions before finally trying the rotation experiment.

Not one cat, however, did return! Romanes used to describe with much amusement the ludicrous nature of the experiment as seen by passers-by.

Darwin was engaged just now (1881) upon his book on earthworms, and writes regarding it:

> Your letter on intelligence was very useful to me, and I tore up and rewrote what I sent you. I have not attempted to define intelligence, but have quoted your remarks on experience, and have shown how far they apply to worms. It seems to me, that they must be said to work with some intelligence, anyhow, they are not guided by a blind instinct.

The following interesting remarks occur in the same letter:—

> Dr. Roux has sent me a book just published by him, "Der Kampf der Theile," &c., 1881 (240 pages in length). He is manifestly a well-read physiologist and pathologist, and from his position a good anatomist. It is full of reasoning, and this in German is very difficult to me, so that I have only skimmed through each page, here and there reading with a little more care. As far as I can *imperfectly* judge, it is the most important book on evolution which has appeared for some time.
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> I do not know whether you will discuss in your book on the "Mind of Animals" any of the more complex and wonderful instincts. It is unsatisfactory work, as there can be no fossilised instincts, and the sole guide is their state in other members of the same order and mere probability. But if you do discuss any (and it will perhaps be expected of you), I should think that you could not select a better case than that of the sand-wasps, which paralyse their prey, as formerly described by Fabre in his wonderful paper in *Annales des Sciences*, and since amplified in his admirable "Souvenirs." Whilst reading this latter book, I speculated a little on the subject. Astonishing nonsense is often spoken of the sand-wasp's knowledge of anatomy. Now will any one say that the Gauchos on the plains of La Plata have such knowledge, yet I have often seen them prick a struggling and lassoed cow on the ground with unerring skill, which no mere anatomist could imitate. The pointed knife was infallibly driven in between the vertebræ by a single slight thrust. I presume that the art was first discovered by chance, and that each young Gaucho sees exactly how the others do it, and then with a very little practice learning the art. Now I suppose that the sand-wasps originally merely killed their prey by stinging them in many places (see p. 129 of Fabre, "Souvenirs," and p. 241), on the lower and softer side of the body, and that to sting a certain segment was found by far the most successful method, and was inherited, like the tendency of a bull-dog to pin the nose of a bull, or of a ferret to bite the cerebellum.

Darwin's attitude on the subject of vivisection is very manifest in several of the letters. As all the world knows, he did not himself practise vivisection, and the probability is that his gentle nature caused him to regard with more than ordinary dislike the necessity for experiments upon animals which might involve pain. But he saw so clearly that no real advance can be made in science without experiment, that he was ready even to come forward as a champion of the cause of physiology. In 1877, when the first antivivisection agitation was at its height, he writes:

> I am inclined to think that writing against the bigots about vivisection is as hopeless as stemming a torrent with a reed. . . . It seems to me the Physiologists are now in the position of a persecuted religious sect, and they must grin and bear the persecution, however cruel and unjust, as well as they can.

And in 1881—

> Do you read the *Times?* As I had a fair opportunity, I sent a letter to the *Times* on Vivisection, which is printed to-day. I thought it fair to bear my share of the abuse poured in so atrocious a manner on all physiologists.

Darwin's regard for physiologists was reciprocated; he and Sharpey were the first honorary members elected by the Physiological Society (of which Romanes was one of the first secretaries), a "mark of sympathy" with which he expresses himself as being "very much gratified." Needless to say Darwin's death was an untold loss to Romanes.

> Even the death of my own father—though I loved him deeply, and though it was more sudden—did not leave a desolation so terrible. Half the interest of my life seems to have gone when I cannot look forward any more to his dear voice of welcome, or to the letters that were my greatest happiness. . . . And when I think how grand and generous his kindness was to me, grief is no word for my loss.

In the words of his biographer—"Thus closed a very significant and important chapter in his life."

Of Romanes' other scientific correspondence, the earliest (1875-76) was with Prof. Schäfer, and related to his experiments upon Medusæ. In these letters he describes many interesting results which he was obtaining, and which were, for the most part, afterwards published in the *Philosophical Transactions.* He had then and always the pen of a ready writer (in hand as well as in tongue), and was besides in the habit of illustrating his descriptions by rough diagrams, so that they are admirably clear and instructive. Nearly all the other letters to scientific men are of comparatively recent date, and deal mainly with the controversies on problems of inheritance (physiological selection and Weismannism) in which, as the readers of NATURE well know, he was so deeply engaged. His paper on physiological selection[1] was read before the Linnean Society in May 1886. It raised a storm of opposition, largely, as he thought, because its opponents would not take the trouble to understand it. To Prof. Meldola he writes:

> Physiological selection seems to have brought a regular nest of hornets about my head. . . . It seems to me that there is a good deal of misunderstanding abroad, due, no doubt, to the insufficiency with which my theory has been stated.

There are also several long letters to Mr. Thiselton-Dyer and Mr. Francis Darwin on the same subject, and a friendly controversial correspondence with Prof. Poulton on the subject of Weismannism. But most of these letters have a more or less continuous thread of argument running through them, and do not lend themselves readily to extracts. The reader who is interested in the controversies is therefore referred to the originals. Even after he had been stricken by paralysis he was not to be deterred from continuing to discuss the problems which most interested him, and he carried on a long correspondence with the Rev. G. Henslow on the subject of the direct action of the environment on plant structures. In his last illness the personal sympathy which he received from his scientific friends much touched him, hard though he felt his fate to be. In a letter to Mr. Thiselton-Dyer, dated September 1893, he writes:

> When one is descending into the dark valley, scientific squabbles seem to fade away in those elementary principles of good will which bind mankind together. And I am glad to think that in all the large circle of my friends and correspondents there is no vestige of ill will in any quarter, unless it be with —— and ——, who both seem to me half-crazy in their enmity, and therefore not of much count.
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> As for "fortitude," sooner or later the night must come for all of us; and if my daylight is being suddenly eclipsed, there is only the more need to work while it lasts. But, to tell the truth, I do not on this account feel less keenly the pity of it. With five boys—the eldest not yet in his teens and the youngest still in his weeks; with piles of note-books which nobody else can utilise, and heaps of experimental researches in project which nobody else is likely to undertake, I do bitterly feel that my lot is a hard one.

Throughout the letters the character of the man comes openly to the surface, and all the world may see in them the simple child-like nature, the unvarying good humour, the gentle disposition, which were combined in him with the highest intellectual attainments; qualities which won for George Romanes the affectionate regard of all who were privileged to know him.

E. A. SCHÄFER.

EARLY LEGENDS AND PREHISTORIC FOLK-LORE.

The Life and Exploits of Alexander the Great; being a Series of Translations of the Ethiopic Histories of Alexander by the Pseudo-Callisthenes and other Writers, with Introduction, &c. By E. A. Wallis Budge, Litt.D. Pp. xv + liv + 610. (London: C. J. Clay and Sons, 1896.)

WITHIN recent years it has been recognised that the legends and epics of ancient peoples are something more than collections of quaint and amusing tales, that they have a scientific value, and that they yield important results when studied, classified, and compared. The sagas of many nations are well known, and have been already subjected to an exhaustive process of inquiry, but those of others have still to be unearthed. Among this latter class the legendary literature of the Ethiopians had until recently to be set, but the volume by Dr. Wallis Budge, the title of which stands above, will go some way to remove the obscurity in which the beliefs and traditions of that nation have been shrouded. Hitherto the Ethiopic literature that has been published, has in the main been biblical and of interest chiefly to biblical students; Dr. Budge, however, has collected a goodly body of Ethiopian traditions from MSS. in the British Museum and in the Bibliothèque Nationale, Paris, two of the former having been among those which were brought to England from the Treasury of King Theodore by the British Army in 1868. The Ethiopic texts of these MSS. have been edited by Dr. Budge, and they have been printed for private circulation by Lady Meux. An English translation and introduction which accompanied the text has, however, been published separately on smaller paper, for the use of those to whom the subject-matter, rather than the text of the MSS., would be of interest; and it is with some of the results to be obtained from a perusal of this latter volume, that we propose in the present article to deal. It is not our purpose, however, to treat the legends from a literary point of view; our object is rather to extract from them such information as will indicate what was the condition of geographical and astronomical knowledge among early Oriental nations, and to notice briefly the stories of heroes and others which grew up when the world was yet in its childhood, and when early man was himself still mystified by the phenomena of nature he beheld around him.

The central figure round which the stories group themselves is Alexander the Great, many of them being based on traditions or stories borrowed originally from other races. For instance, the book comprises the Ethiopic version of the Pseudo-Callisthenes, which has found its way into many languages; extracts from the Ethiopic versions of larger historical works by Al-Makîn and Abû Shâker, Arabic historians of the thirteenth century; and an extract from the Ethiopic version of Joseph ben-Gorion's "History of the Jews." The book also contains a short account of Alexander's death, and the utterances of the sages thereon; the "Christian Romance," which is probably an original Ethiopian work; and an account of the "Vision of Abbâ Gerasimus." None of the Ethiopic MSS. which contain these works is actually older than the seventeenth century; they are, however,

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[1] An admirable epitome of this theory (and of each one of the scientific subjects which chiefly engaged Romanes' attention) is given by his biographer in the book before us.